

tree stem, play a conspicuous part, after having been brought, fresh painted, from the village to the feasting place. Sometimes there is even a migration of the revellers into another village, which generally results in a kind of sham fight between the two parties.

With regard to Betel chewing and tobacco-smoking, both these habits are very prevalent, and freely indulged in by the people inhabiting Maclay coast. Nevertheless, the *Areca* palm is by no means abundant on the coast, being, with the exception of the "Archipelago of Contentment" and a few other spots, but seldom found in the villages. The process of Betel-chewing is as follows: The *Areca* nut having been first partly masticated, is then mixed with a little powdered lime, which is carried in a special box formed from bamboo or a calabash gourd, and, after being rolled in a Betel<sup>2</sup> leaf or two, is placed between the teeth and chewed. Although the tobacco plant, here called *Kas'*, is much cultivated, and flourishes well along the whole coast, the American tobacco, pressed flat into cakes, which Maclay had brought with him, was so much liked and prized, that he contributed a portion at almost every *Ai* feast. This, after being separated into its component leaves, was dried over a fire, torn into little shreds, and then rolled into cigarettes in green leaves, also previously dried at the fire. A single cigarette makes the round of a number of smokers. In smoking the Papuans swallow the smoke, and blow the rest through the nose.

As the *Keu* has soporific qualities, the Papuans have devised a means of keeping any one who has succumbed to its influence in a wakeful condition. The victim to its power betakes himself to a friend, who with a stalk of grass tickles the cornea and conjunctiva of his eyes until they become full of tears. This is repeated until the patient declares that he feels no longer sleepy. This operation is regarded as a very pleasant one, but "whether it always succeeds," remarks M. Maclay, "is another question."

J. C. GALTON

### EPIDEMICS<sup>3</sup>

WE are now entering on our thirtieth session, and, I trust, with reason to believe that our progress is satisfactory, and our work such as to prove that the Society is fulfilling the main object for which it was founded. Though not one of the largest, it is certainly not one of the least active or important among the medical societies of the metropolis, whilst the cosmopolitan range of subjects embraced within the scope of its inquiry renders its proceedings of far more than mere local interest.

The *raison d'être* of this Society is the investigation and development of our knowledge of disease in motion. It involves much, for any disease where it spreads, whether among the people of a house, a ship, a village, a city, a province, or a continent, is an epidemic, and comes within the scope of our inquiry. Dysentery and malarious fevers are typically endemic diseases, but in India they may and do at times assume a dangerously epidemic character. But it is not meant that our inquiries should be restricted to mere epidemicity alone; we cannot advantageously study one phase of the natural history of disease and exclude others. There is so much in etiology, semeiology, and pathology both human and comparative that concerns our department of research that we may not ignore the means by which we gain the most important of all information to the epidemiologist—namely, the means of discriminating one form of disease from another. Therefore, though our proceedings will naturally refer mainly to

epidemic disease, we shall thankfully receive and carefully consider all information that may tend in any way to throw light on the causal relations, and on the influences exercised by climate, season, locality, food, and occupation on the genesis and dissemination of all diseases, not excluding those of the lower animals, nor even of plant life. I might illustrate this by referring to the importance of discriminating between the different forms of fever that occur in India. It is well known that the greatest proportion of mortality in India is ascribed to fevers. The highest death-rate<sup>1</sup> was registered in Bombay, where it equalled 20·82 per 1000; in Madras the deaths of 469,241 persons gave a death-rate of 10·08 per 1,000. Both here and at Bombay, however, the mortality recorded was greatly aggravated by famine.

There is no doubt that, under the heading Fever, many deaths from other causes are recorded, and we may probably refer a large proportion of them to diseases of an inflammatory character affecting the thoracic or other viscera, or to complications involving inflammatory action elsewhere. In a vast country like India, where the population is so extensive, and the means of registration of necessity limited, often not under medical supervision at all, it is not to be expected that greater accuracy can be ensured; but, were it possible to discriminate among the various forms of disease returned as causing the mortality by fever, we should have a very different result from the present. One can hardly refer to this subject without expressing admiration at the great progress that has been made of late years in registration under the direction of the sanitary authorities of India, and confidence that it will continue to improve, and render the statistics more valuable even than they are now. Of course, where the registration of death is not subject to medical definition discrimination between the various forms of fever or other death-causes could hardly be expected; and therefore the example I have just given is hardly so good an illustration of what I refer to as typhoid, for which we have accurate medical statistics of our European troops in India. It is within my recollection that attention was first called to the existence of this form of fever in India, and yet there can be, I suppose, no doubt that it has always been there. It soon became generally recognised as a new discovery in India, and people wondered how it had escaped observation hitherto, whilst some perchance regarded it as a new disease. But it was just this power of discriminating observation that is so rare and so valuable that had been wanting; it was this that, exercised by Budd, Jenner, Stewart, Murchison, and others after them, established a new era in the nosology of fevers in England; and it was this, that, a little later, in India, discriminated between certain forms of remittent and enteric (*i.e.*, between malarious and specific) fevers, and that will, I hope, ere long further discriminate and rearrange the nosology of fevers in India and the tropics.

Now typhoid or enteric fever is an important cause of mortality among our young European soldiers in India; and it raises or suggests questions of great importance in regard to them—such, for example, as the right age, time, and seasons for sending them to India; to say nothing of the hygienic questions as to sanitary measures arising out of its causal relations. The Sanitary Commissioner's Report of 1877 says that out of 233 cases of typhoid, 92, or 39 per cent., proved fatal; the admission rate being 4·1 per 1,000 of strength. It moreover appears that 2·45 per cent. occurred at or under twenty-four years of age; 1·55 at twenty-five to twenty-nine; 0·99 at thirty to thirty-four; and a few or none above that age; showing that the disease tells most severely on the younger men—in this respect resembling typhoid in England. Again, Bryden, in his Report of the Statistical History of the European Army in India up to 1876 (published 1878),

*Vide* Report of Sanitary Commission of the Government of India for 1877.

<sup>1</sup> *Pinang* in the Malay language.

<sup>2</sup> *Siri* in Malay.

<sup>3</sup> Abstract of Presidential Address at the Epidemiological Society, delivered on November 5, 1879, by Sir Joseph Fayrer, K.C.S.I., LL.D., M.D., F.R.S.

says: "It has no geography; and it is a matter of popular observation that no regiment or battery escapes enteric fever *in the first year*, whatever cantonment of India may be selected." "Out of seventy-three bodies of men two regiments and seven batteries only returned no case of enteric fever in the first year." And he gives the following analysis of 368 deaths that occurred between 1823 and 1876:—

Ages.				Total deaths.
24 and under	...	...	...	255
25 to 29	...	...	...	90
30 to 34	...	...	...	17
35 to 39	...	...	...	4
40 and upwards	...	...	...	2

Seventy-five of these deaths occurred within three years after landing in India, and 94 per cent. of the total were among men under thirty years of age. In a memorandum received only a few days ago Bryden says, out of 132 deaths from enteric fever in 1878, 90 occurred in men who had been under twenty-two months in India. All this shows that youth and the first year of service in India are the great predisposing causes.

Now is this the same disease as that which might be contracted in London, Dublin, Windsor, or elsewhere in a town or barrack? from a watercloset, drain, sewer, well, or, it may be, from a milk-can? I have little doubt that very frequently it is exactly identical; but I believe also that perhaps as frequently, or more so, it is not; and this, I believe, not in regard to young European soldiers only, but of the whole population of India. In short, I am, and long have been, of opinion that a form of fever exactly like European typhoid, except in its etiology, exists in India and other hot and malarious countries; and that it is due to climatic causes, not to filth or specific causes such as give rise to it in England and elsewhere, and recent reports from India and other parts of the world seem to show that this view is gaining ground.

Be it clearly understood, however, that I do not for a moment dispute the existence of genuine *filth* typhoid in India. The official returns, which cannot be gainsaid, and my own experience alike leave me in no doubt about it. But I do believe that many cases now recorded, and rightly recorded, as typhoid in India, are not caused by the same specific agency as that which gives rise to typhoid here, and also in India, where the necessary conditions are present. There are, in fact, two, perhaps more, forms of typhoid, or perhaps, I should say, different causes for a disease presenting the same phenomena, one being specific, the other climatic; if so, it is obviously very desirable to discriminate between them—not merely as a matter of nosological or pathological interest, but in regard to the hygienic measures that are necessary in either case. This question is of great interest to epidemiologists, and is just one of those subjects that should come within the scope of our inquiry. A remarkable instance occurred near London lately, where an *epidemic* of typhoid was traced to its origin along the track of a particular water-supply by a distinguished member of our Society. Our lamented colleague, Murchison, also traced a similar outbreak to contamination of milk by polluted water. The explanations of these outbreaks were exactly in accordance with these views, no doubt rightly held in Europe. Typhoid in India, however, would not always be similarly explained.

We know but little of the nature of the causes of epidemics such as cholera, or of malarious diseases, but we know something of what may be expected in regard to them, when and how they will appear, and how sanitary work may be made most effectually to avert, prevent, control, mitigate, or remove them; nor is it too much to say that we hope, if not to banish or stamp out, at all events still further to mitigate

their ravages. This, I think, is warranted by experience, for certainly the past twenty years have witnessed great progress, and we already see that not only is the value of life increasing, but that the virulence of, and mortality caused by, epidemic disease are being controlled, whilst the vexatious and purposeless restrictions and restraints of quarantine are diminished if not altogether removed, in our dominions at least. Science that has enabled us to reduce the death rate among our troops from 17·9 to 8·56 per 1,000 in Europe, and from 69 to 17·62 per 1,000 in India, speaks for itself, and were there no other result this alone is a triumph such as has been achieved by no other department of knowledge. Pray do not suppose that I claim all this for Indian workers. We all know that these great steps in social and sanitary progress began here, but I do say that the torch then lighted has since been worthily and firmly borne in Indian hands.

This Society, young as it is, can remember the commencement of systematic sanitary work in the East, and may claim some share in the origination of the good work; for among its earlier members were some who advocated the study of epidemiology and hygiene in India. I cannot now stay to dwell on this, but I may say briefly in regard to its progress and work that until the sanitary department was formed, less than twenty years ago, comparative, I might almost say complete, ignorance on the subject of epidemics, and of the diseases that prevailed among the people, existed. An epidemic might carry off thousands, but we knew not where it commenced, where it ended, or what area it occupied. Now, thanks to the continued and careful statistics, we know all that and more, with fair accuracy, and are gradually collecting facts which make the study of epidemiology possible. Before organised sanitary work in India began our knowledge of the general population—nay, even of the European troops and prisoners—was most imperfect. Now, thanks to that department, and especially to Bryden, whose name cannot be too prominently mentioned in connection with the subject, we have, in his most elaborate and valuable statistical reports, facts and figures, as well as deductions, which deserve the closest attention. We have, in short, the most complete details of sickness and mortality in all classes over the whole of India. It is impossible, however much any one may differ from his conclusions, not to recognise the great value of his work, for these reports contain a vast and continued array of authenticated facts which will serve as a mine of information to epidemiologists; and there is every reason for believing that it is but an earnest of more, for if, as Bedford said, the circumstances of India were such as to favour the acquisition of knowledge in 1850, when the precise conditions of life of the population generally, and even of our troops and prisoners were imperfectly known, what must it be now when a system of observation, carried out by a body of trained observers under a head such as he contemplated, is in full and daily improving operation? One can only wish that, considering the magnitude of the work, it were more extensive still, and that observations, already of the greatest value, could be rendered still more so by being concentrated on certain limited areas so as to enable the inquiry to extend to details with a precision that at present can hardly be practicable. The results of epidemiological knowledge and sanitary work are seen in the effect already produced in reducing the mortality from cholera and other epidemics, and from malarial fevers. For instance, among our European troops, the circumstances of which are well known, there has been the following alteration in the general death-rate:—

1861 to 1865	...	...	9·02 per 1,000.
1865 to 1870	...	...	6·98 "
1870 to 1875	...	...	3·23 "
1875 to 1876	...	...	2·3 "
1876 to 1877	...	...	·84 "

And it can be shown from the same source (Bryden) that there is a similar reduction in the death-rate among native troops; and that, during the great cholera epidemic that prevailed in 1876, the death-rate among the civil population being 12·12 per 1,000 (it was the famine year), that of the European troops was 1·75 and of the native army 2·2 per 1,000. Also that in the death-rate of that great scourge of India—fever—there has been an equally happy result, as shown by the death-rate of a period of nine years—from 1868 to 1877—in the Bengal and N.W. Provinces gaols, as against a mean, in nine years ending 1867, of 22·41.<sup>1</sup>

	Per 1,000.		Per 1,000.
1859 ... ..	13·76	1868 ... ..	2·84
1860 ... ..	49·19	1869 ... ..	4·57
1861 ... ..	38·14	1870 ... ..	6·20
1862 ... ..	30·81	1871 ... ..	5·81
1863 ... ..	25·44	1872 ... ..	1·92
1864 ... ..	29·96	1873 ... ..	1·56
1865 ... ..	7·65	1874 ... ..	2·67
1866 ... ..	5·23	1875 ... ..	3·50
1867 ... ..	3·12	1876 ... ..	1·26
Mean ... ..	22·41	Mean ... ..	3·29

In India, as elsewhere, the purpose of epidemiological study is to observe accurately and to interpret the import of the facts—*i.e.* if possible, to elucidate the laws of which they are the expression,—and thus to form a scientific basis on which to direct sanitary work, which itself is the practical outcome of such observation, and concerns itself but little with theories. Those who have to do with it know how difficult it sometimes is to obtain *reliable* observations, such is the difficulty of excluding the bias of preconceived theory of the nature of the subject under investigation. Our Society, however, must hold the balance, and deal with theories as well as facts. No doubt the explanations sometimes are conflicting. Happily, in the practical mode of dealing with the question, there is not much conflict, and it is remarkable how little different is the action of those who hold opposite opinions on the causation of disease. As to the different views that are held in regard to fundamental questions regarding the genesis and diffusion of disease let us hope that we may, perhaps, here contribute something towards their adjustment.

In illustration of the state and progress of epidemiology in India I might select the history of any of the great epidemics that have occurred of late years, but it would be impossible, in the short space of time at my disposal, to do this completely. I shall, therefore, confine myself to a few remarks on cholera, as it, though far from being the most destructive, is the epidemic to which most interest attaches.

The theory that cholera is purely of Indian origin, and that wherever it may appear it is to be traced back ultimately to the delta of the Ganges, is disputed by some who see in history evidence that it had long been existing in other parts of the world, and that it was described by the earliest writers—Sanskrit, Greek, and Arabian. I may here just remark that the name “haida,” or “haiza,” used by Rhazes nearly 1000 years ago in describing the symptoms of cholera, is the same as that applied to it now by every Hindustanee-speaking native of India. As to its presence in India, from earlier periods we have descriptions by Correa, d’Orta, Bontius, and others, beginning from 1503. An epidemic of it in and about Goa in 1543, for example, is described by d’Orta, giving all the characteristics that distinguish an epidemic now. He calls it “moryxy” and also “haiza.” It is described by a continuous chain of writers as occurring in various parts of India, in the interior as well as on the coast, up to the seventeenth century, when, after being quiescent during the later part of the eighteenth and early part of the nineteenth century,

it broke out with great virulence in Bengal, and has remained there ever since, in what Bryden calls its endemic area, whence it spreads according to certain laws, which are being worked out with admirable patience and intelligence by this distinguished statistician. I cannot now trace the history of cholera in either East or West, nor refer even to the numerous authors who have described it. For full particulars on this subject I refer you to Inspector-General Dr. J. Macpherson’s learned work, “The Annals of Cholera.”

I need hardly insist that cholera is not a new disease, or that it did not, as by some supposed, make its first appearance as an epidemic in Jessore, in Bengal, in 1813; though no doubt since that period it has been more closely investigated and described. All seems to show that it is the same now as formerly, and that though we have gained much knowledge of its natural history of late years, yet we are as ignorant as our predecessors of its real nature. We have, thanks to sanitary measures, disarmed it of some of its terrors, and have diminished the mortality it caused; but as to treatment we have gained but little, though the empiricism of to-day is more scientific than it was in former days. We do not now burn our patients on the soles of the feet, tie ligatures round their limbs, or have recourse to other senseless barbarities; for we find that simpler and more rational methods are of greater avail, more or less according to the period of the epidemic at, and the promptitude with, which the remedies are applied. But we have learned that local causes have a potent influence, and that cleanliness, good air, *pure water*, and free ventilation are all powerful opponents of cholera; that we can predict its appearance and avoid it in certain places; and that it is not to be controlled by quarantine or sanitary cordons. And from the earnestness and intelligence with which the subject of its etiology is pursued, it is not improbable that sooner or later it too will be made out. We shall then be in a position to say, not only what it does or will do, but what it *is*. Meanwhile we must go on observing and investigating. It is satisfactory to know that we are daily learning, practically, better how to deal with it, and how to modify its cyclical intensity and avoid its ravages. For my own part, until I know something more of the *nature* of the cause—be it a material poison, aerial or telluric, a miasm, or a dynamic agency that so perturbs the vital energy, I cannot see my way to formulate a definite theory either of the nature of its origin or the method of its diffusion. I find the highest authorities at complete variance on the subject, and reposing faith in theories diametrically opposed to each other. Some explain all the phenomena by contagion—*i.e.*, communicability in some way of a *matieres* or germ from one person to another. Cholera, they say, is the result of infection by a poison derived from the intestines, and water or air, but especially water, is the channel through, by, or in which the infective material is intensified and conveyed. These arguments are supported by an abundant array of facts, and have been maintained by men whose very names carry conviction. Others reject altogether this explanation; they insist that local influences are all-important, and deny that the spread of cholera is due to human intercourse, that there is any poison transmitted by the excreta, or that the disease is in any way communicable from one person to another. They admit the existence of a poison of some sort—a miasm or an influence, though of its nature they are ignorant. It is a subtle thing that travels in certain directions in obedience to certain laws, is influenced by atmospheric and telluric conditions, and where it finds certain local conditions, and the people prepared by them to submit to it, there cholera will prevail. They deny the efficacy of any enteric or specific poison in the water to produce it, though they attach the greatest importance to the purity of water from *all* organic contamination, impure water being one

<sup>1</sup> Bryden’s Report (1876), p. 157.

of the local conditions which, if added to crowding, filth, or other insanitary conditions and want of proper ventilation, is that of *all others* which favours cholera. Such are the principal theories of the disease as they are supported by fact and argument which appear convincing. Europeans, Americans, and some authorities in India maintain the contagious view, whilst high authorities in India adhere to the opposite theory, and declare roundly that the facts of cholera, in India at least, are altogether opposed to the contagion theory.

On the question of importation of cholera in the Punjab epidemic (1875-6) and its spread by human agency, Dr. Bryden, in his report, p. 308 (1876, published 1878), says:—"The assertion amounts to this. The Punjab is divided into thirty-two districts. Cholera was introduced into seventeen of these in 1875, and therefore it spread. Cholera was *not* introduced into fifteen districts, and therefore did not appear, or, if it was introduced, influences of which we know nothing stopped its propagation in these districts. Unknown causes prevailing in the one-half of the area are presumed to influence the human system so that it is capable of receiving cholera, and in the other to act universally as an antidote even to a cholera poison when introduced. This is what is offered as antagonistic to the theory which asserts that cholera is air-borne and is as far-flying as are the limits of natural areas. Primarily, these theories cannot be reconciled; the one or the other is false. If cholera is spread only by the human being, the theory which shows it to be air-conveyed is untrue; if cholera is spread solely as an aërial miasm, then the theory which recognises only the effects of human intercourse is unfounded. The most that is admitted by the advocates of the human theory is, that the subjection of cholera to meteorological agencies is absolute, and that these influences can, and do operate so as to do away altogether with the effects of the poison, although imported. They do not recognise the entity *minus* the human being. The antagonistic theory, while holding, as opposed to demonstrable fact, the statement that cholera moves only by human agency, may, if necessary, be extended in its scope so as to embrace the other; that is to say, the inquiry is left open as to whether or not the cholera entity, after being aërially distributed, may be subsequently propagated or spread by man. In the theory which connects cholera solely with man there is no such extensibility. Human intercourse must explain every fact of spread and propagation, and nothing is left to the play of natural agencies. Human intercourse, giving the widest scope to the signification of the term, cannot pretend to account for any fundamental phenomenon displayed during the progress of epidemic cholera; and, therefore, I assert the theory to be radically untrue as applied to the behaviour of cholera in India. I do not say that the above statement will hold true all over the world; and, even as applied to India, the theory does not preclude the possibility that cholera may be conveyed by the human being. Naturally the recoil is to the opposite extreme. The bold statement that cholera is never spread over an area unless human agency intervenes, is apt to be met by the equally dogmatic statement that cholera is as pure a miasm as malaria, and as little amenable to the control of man.

In the present state of our knowledge we can only be guided by the inferences from well-ascertained facts and such laws as we have ascertained to be in constant operation, watching and carefully observing until we may, were it only by a process of exclusion, arrive at some deeper knowledge still. "So far," says the chief sanitary authority in India, "the history of cholera is full of enigmas and seeming contradictions, and though we have of late years collected many valuable data, and understood the importance of studying them on a broad basis, we know no more of the exact cause of the disease than our grandfathers did. We know that, whatever the cause may be, it flourishes in the midst of

insanitary conditions of dirt and overcrowding, and especially of impure water, impure from whatever cause; we know that it is liable to occur under certain conditions, and at certain times and seasons, and we should endeavour to extend that knowledge, and hope, as we do so, to arrive at the precise nature of the disease itself." But it may be well to remember that "expenditure of public money must take place only on observed facts and experience," not in accordance with theories. "It would be prejudicial to real sanitary work if opinions which have been promulgated in some parts of India, as to the cause of cholera being due *solely* to the state of the water-supply, were to take root."

It is most important that we should arrive at some definite conclusion as to the real nature of the disease, for it is impossible but that our conceptions on this subject must influence the sanitary measures that deal with it, and I believe the question must find its final solution in India, where the disease is always present in its endemic and seldom-absent from the epidemic areas. The highest authorities—for both I have the greatest respect—differ *toto calo* on this subject. Is it that they are both right, though seemingly so opposed in their views? Like the knights who fought about the silver and golden sides of the shield, will they not change places, and find why they differed?

I would venture to suggest that in India the inquiry might be pushed with more detail in regard to individual cases and outbreaks in certain limited areas; and that, on the other hand, one or more epidemiologists of European fame should be deputed to visit India and study cholera with the eminent men who have devoted so many years of close attention to it there. It might be, I believe it would be, that mutually they would gain from each other, and that those who went out would find their own views confirmed as to the disease in Europe—modified as to what they deemed it to be in India. Of one thing I am convinced, that simple truth is the object of their search; and I feel sure that from such combined action the greatest benefit would result.

#### POPULAR NATURAL HISTORY<sup>1</sup>

JUDGING by the continuous stream of popular "Natural Histories," the demand for such books must be great. Messrs. Cassell have already published many volumes of the kind, some dealing with the whole animal kingdom, some with a single class; they are now bringing out a series of volumes under the editorship of Dr. Martin Duncan, and in the mean time they present us with a single volume of a still more popular character by Dr. Perceval Wright. This book, as we are told in the preface, is intended for that large class of readers who, while they take an intelligent interest in the study of natural history, have but little taste for the technical details which would naturally form the bulk of a scientific manual on the subject. For this reason the space devoted to the several orders is roughly proportioned to the amount of interest generally felt in them. The mammalia occupy more than a third of the volume, the remaining orders of the vertebrata about an equal space, while the whole of the invertebrates are compressed into the space that remains—about one-fourth of that occupied by the vertebrates. The author tells us that his aim has been "to compile a story-book about animals, and at the same time in some degree to write a scientific manual." This is undoubtedly a difficult thing to do, and to do it thoroughly and in the best style would be a fitting life-work for a great naturalist. It would have to be done as a labour of love, not to the order of a publisher; and the

<sup>1</sup> Animal Life: being a Series of Descriptions of the Various Sub-kingdoms of the Animal Kingdom. By Perceval E. Wright, M.A., M.D., &c., Professor of Botany in the University of Dublin. With Illustrations. (Cassell, Petter, Galpin, and Co., London, Paris, and New York.)